ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 MAY 30—JUNE 5

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on May 30

Sun rises, 3h. 52m.; souths, 11h. 57m. 15·7s.; sets, 20h. 2m.; decl. on meridian, 21° 48′ N.: Sidereal Time at Sunset, 12h. 35m.

Moon (New on June 2) rises, 2h. 37m.; souths, 9h. 20m.; sets, 16h. 14m.; decl. on meridian, 7° 36′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	3 20	10 58	18 36	17 39 N.
Venus	2 22	9 5	15 48	7 43 N.
Mars	11 57	18 35	1 13*	6 44 N.
Jupiter	12 57	19 15	1 33*	2 50 N.
Saturn	5 50	14 1	22 12	22 46 N.

* Indicates that the setting is that of the following morning.

May	h.	
30	1	Venus in conjunction with and 1° 18′ north of the Moon.
June		
4	10	Saturn in conjunction with and 4° 1′ north of the Moon.

Variable Stars

Star	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·2	81 16 N.	May 30,	2 57	*m*
			June 3,	2 37	*m*
S Cancri	8 37·4	19 17 N.	May 31,	22 12	*m*
S Bootis	14 19·1	54 20 N.	June 2,		*M*
δ Libræ	14 54·9	8 4 S.	May 30,	1 16	*m*
U Coronæ	15 13·6	32 4 N.	,, 31,	3 34	*m*
R Herculis	16 1·1	18 41 N.	June 2,		*M*
U Ophiuchi	17 10·8	1 20 N.	May 31,	3 2	*m*
			and at intervals of	20 8	
X Sagittarii	17 40·4	27 47 S.	June 5,	2 20	*M*
W Sagittarii	17 57·8	29 35 S.	,, 2,	21 30	*m*
U Sagittarii	18 25·2	19 12 S.	,, 1,	21 40	*M*
β Lyræ	18 45·9	33 14 N.	,, 1,	2 25	*M*

M signifies maximum; *m* minimum.

Meteor Showers

The shortness of the nights at this season of the year greatly interferes with meteor observation, and no great periodical shower occurs at this time. Meteors from the following radiants amongst others have been observed. Near Cor Caroli, R.A. 206°, Decl. 39° N.; near ε Coronæ, R.A. 240°, Decl. 25° N.; near β Lyræ, R.A. 280°, Decl. 29° N.; near κ Cephei, R.A. 289°, Decl. 80° N.

Stars with Remarkable Spectra

Name of Star	R.A. 1886·0 h. m. s.	Decl. 1886·0 ° ′	Type of spectrum
407 Birmingham	17 14 2	2 15·4 N.	III.
D.M. + 17° 3241	17 20 49	17 1·1 N.	III.
Arg. Oeltzen 17681	18 1 7	21 16·3 S.	Bright lines
D.M. + 43° 2890	18 3 21	43 26·3 N.	III.
458 Birmingham	18 38 51	36 50·7 N.	IV.
464 Birmingham	18 43 43	8 2·0 S.	IV.
δ^2 Lyræ	18 50 30	36 45·2 N.	III.
R Lyræ	18 51 52	43 47·6 N.	III.
222*c* Schjellerup	18 58 18	5 51·2 S.	IV.

GEOGRAPHICAL NOTES

In his presidential address at the annual meeting of the Royal Geographical Society on Monday, the Marquis of Lorne, referring to the matter of geographical education, said that the interest excited in the subject by the Society's recent action has been so great, and the expectation that the Society will continue it by taking some positive steps towards encouraging improvements in the position of geography in schools and Universities is so general, that the Council have felt encouraged and indeed bound to carry the scheme further. The Educational Committee of the Society therefore made certain suggestions to the Council, which are now under consideration, and will probably be adopted. The principal of these suggestions relates to the appointment of a lecturer in geography to deliver courses where the Council may direct. In this matter the Council will take suitable steps to obtain the co-operation of the Universities of Oxford and Cambridge. In order still further to encourage the scientific study of geography at the Universities, the Committee suggest that a prize or travelling scholarship be given every alternate year to a student who has shown marked ability in geographical subjects, and who may desire to visit one of the less-known districts of Europe, or the Mediterranean or Black Sea shores, and any results be communicated to the Society. One or other of the annual grants which are at the Society's disposal might be devoted to this purpose. Another suggestion is aimed at reaching the intelligent middle and working classes through the medium of the University Extension Scheme. For this purpose a small annual grant is proposed. Another is that a medal be given by the Society to the student reported by the examiners to have done best in physical geography in the first part of the Natural Sciences Tripos (Honours Examination). And finally, in order that all classes of schools may be reached, it is proposed that prizes be offered for competence in geography to the students at the various training-colleges. "Here we reach the fountain-head of education, and if we can secure adequate attention to geography in the institutions which send forth yearly troops of teachers to our Board and elementary schools, the Society will have accomplished much. It is evident, then, that the Society has already accomplished a great deal. The mind of the public has been aroused and greatly enlightened on the subject; our best schools and Universities have expressed their willingness to co-operate as far as possible in carrying out improvements; and there can be little doubt that our proposed further action will bring results which the Council and all interested in geography have long desired."

In a paper contributed to the *Bulletin* of the Moscow Society of Naturalists (1885, No. 2) M. Smirnoff continues his most valuable delimitation of the vegetable zones of the Caucasus, which forms an introduction to a flora of the vascular plants of the region. He subdivides Transcaucasia into several zones, the central zone extending east to the meridian of Shemakha. This limit does not correspond to any orographical features, but separates from the remainder of Transcaucasia the region subject to the influence of the Caspian Sea. The littoral of the Black Sea in Transcaucasia is distinguished from the rest of the region by high winter temperatures. As far as lat. 44° N., and even at an altitude of 150 metres, the winter is as mild as in Provence or in Central Italy, only the setting in of warm weather in spring being a little later. But as soon as the chain is crossed we find a rapid decrease of the winter temperature, so that Yekaterinodar, on the northern slope of the chain, although only 20 miles further to the north and 1° of longitude more to the east than Novorossiysk, has an average temperature during December and January 4° lower. Baku and Lencoran have winters very much like those of the Venetian littoral, but Derbent shows a sudden decrease of 3°·5 of temperature in January, probably due to the influence of ice gathering in the northern part of the Caspian Sea, while Petrovsk, only 70 miles further north on the same coast, shows a further sudden decrease of temperature in January. The ranges of the monthly average temperatures of different places show a still greater difference of climate. Thus, on the Black Sea coast, south of lat. 44° N., and even at Kutais, the difference between the warmest and the coldest months does not exceed 18° to 19°; it is the same as at Trieste and Athens. But in Ciscaucasia it reaches 25°, and on the Caspian littoral it varies from 22°·4 at Baku to 29°·8 at Petrovsk. It is still greater on the Armenian plateau (30° to 35°); while in Central Transcaucasia it is generally less than 25°, and rapidly diminishes with the altitude of the place, reaching no more than 19°·5 at Shusha. These few data, together with a map of isotherms prepared by M. Smirnoff, give a broad general idea as to the climatic conditions of the Caucasus, and the consequent distribution of different regions of vegetation through the country.

TELESCOPIC OBJECTIVES AND MIRRORS: THEIR PREPARATION AND TESTING[1]

It would probably lend an additional interest to a technical subject such as I have to bring before you to-night, could I preface my description of the processes now employed in the construction of telescopic objectives by a short historical account of what has been attempted and achieved in the past, but time will not permit.

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[1] Lecture given at the Royal Institution on Friday, April 2, 1886, by Mr. Howard Grubb, F.R.S., F.R.A.S.

A very few words, however, on the history of glass manufacture are necessary.

As I pointed out last Saturday afternoon, Dollond's brilliant discovery, which afforded a means of achromatising objectives, rendered possible their construction of greater size and perfection than formerly, provided suitable material could be obtained. But the chromatic errors being removed, faults in the material hitherto masked by them were detected, and it was not until after many years that Guinand, a lowly but gifted Swiss peasant, succeeded in producing glass disks of a considerable size and free from these defects.

The secrets of his process have been handed down in his own family to M. Feil, of Paris (one of his descendants), and also through M. Bontemps, who for a time was associated with Guinand's son, and afterwards accepted an invitation from Messrs. Chance Bros. and Co., of Birmingham, to assist them in an endeavour to improve that branch of their manufacture. Only these two houses, so far as I am aware, have succeeded in manufacturing optical disks of large size.

Testing of Optical Glass.—Let me here say a few words respecting the testing of optical glass; I mean of the material of the glass, quite apart from the optician's work in forming it into an objective. When received from the glass manufacturer it is sometimes in this state, roughly polished on both sides, and sometimes in this, in which as you see there are small windows only, facets as they are called, polished on the edges. In case of lenses for telescopic objectives, it is always well to have them roughly polished on the sides, to avoid the chance of having to throw away a lens after much trouble and labour has been spent on it.

There are only three distinct points to be looked to in the testing of optical glass:—(1) general clearness and freedom from air-bubbles, specks, pieces of "dead metal," &c.; (2) homogeneity; (3) annealing.

The first is the least important, and needs no instructions for detection of defects, any one can see these. The second is much more important, and much more difficult to test.

The best test for homogeneity is one somewhat equivalent to Foucault's test for figure of concave mirrors.

The disk of glass should be either ground and polished to form a convex lens, or if that be not convenient, it should be placed in juxtaposition with a convex lens of similar or larger size, and whose excellence has been established by previous experience.

The lens or disk is then placed opposite some small brilliant light, a small gas flame generally suffices, and at such a distance that a conjugate focus is formed at other side and at a convenient distance. When the exact position of this focus is found, the eye is placed as nearly as possible so that the image of flame is formed on the pupil. On looking at it with the eye in this position, the whole lens should appear to be "full of light"; but at the slightest movement to one side the light will disappear and the lens appear quite dark. If the eye be now passed slowly backwards and forwards between the position showing light and darkness, any irregularity of density will be most easily seen.

Of course, like everything else, some experience is necessary.

The rationale of this is very obvious. When the eye is placed exactly at the focus of a perfect lens, the image formed on the pupil is very small, and the slightest movement of the eye will cause the light to appear and disappear. If the eye be not at the focus, the pencil of light will be larger, and consequently it will require a much greater movement of the eye to cause the light to disappear. Now if any portion of the lens be of a different density to the general mass, that portion will have a longer or a shorter focus; consequently while the light flashes off the general area of the lens quickly, it still remains on the defective portions.

By imitating this arrangement and substituting a camera for the eye and forming the focus of a small point of light on the stop of the lens, I have succeeded in photographing veins in glass, and sometimes have found this useful as a record.

The third point—that of proper annealing—is easily tested by the polariscope.

For small disks the usual plan is to hold them between the eye and a polarising plane, such as a piece of glass blackened at back or a japanned surface, and look at them through the facets, using as an analyser a Nicol prism.

Larger sizes, which are polished on the surfaces, can be more easily examined. It is difficult to describe the appearances, but I will put a few disks into the lantern polariscope and endeavour to point out what amount of polarisation may safely be permitted in disks of glass to be used for objectives.

The composition of metallic mirrors of the present day differs very little from that used by Sir Isaac Newton. Many and different alloys have been suggested, some including silver or nickel or arsenic; but there is little doubt that the best alloy, taking all things into account, is made with 4 atoms of copper, and 1 of tin, which gives the following proportions by weight: copper, 252, tin, 117·8.

Calculation of Curves.—Having now obtained the proper material to work upon, the first thing necessary is to calculate the curves to give to the lenses, in order that the objective, when finished, may be of the required focus, and be properly corrected for the chromatic and spherical aberrations.

As this lecture is intended to deal principally with the technical details of the process, I do not intend to occupy your time for more than a few moments on this head, nor indeed is it at all necessary. In my lecture last Saturday I explained the principles of achromatism, and in many published works full and complete particulars are given as to the calculation of the curves—particulars which are sufficient, and more than sufficient, for the purpose.

Much has been discussed and written concerning the calculation of curves of objectives, and much care and thought has been bestowed by mathematicians on this subject, and, so far as the actual constructors are concerned, a certain amount of veil is thrown over this part of the undertaking, as if there were a secret involved, and as if each had discovered some wonderful formulæ by which he was enabled to calculate the curves much more accurately than others.

I am sorry to have to dispel this illusion. Practically the case stands thus. The calculation of the curves which satisfy the conditions of achromatism and desired focus is a most simple one, and can be performed by any one having a very slight algebraical knowledge in a few minutes, provided the refractive indices and dispersive power of the glass be known. Both Messrs. Chance and Feil supply these data quite sufficiently accurately for small-size objectives. Speaking for myself, I am quite content to take the figures as given by these glass manufacturers for any disks up to 10 inches in diameter. If over that size, I grind and polish facets on the disk and measure the refractive and dispersive powers myself.

The calculations of the curves required to satisfy the conditions of spherical aberration are very troublesome, but fortunately these may be generally neglected.

Some years ago the Royal Society commissioned one of its members to draw up tables for the use of opticians, giving the curves required to satisfy the conditions of both corrections for all refractive and dispersive indices.

A considerable amount of labour was expended on this work, but in the end it was abandoned, for it was found that the calculation of these curves was founded on the supposition that all surfaces produced by the opticians were truly spherical; while the fact is, a truly spherical curve is the exception, not the rule. The slightest variation in the form or figure of the curve will produce an enormous variation in the correction for spherical aberration, and it was soon apparent that the final correction for spherical aberration must be left to the optician and not to the mathematician. *Object-glasses cannot be made on paper.* When I tell you that a sensible difference in correction for spherical aberration can be made by half an hour's polishing, corresponding probably to a difference in the first place of decimals in radii of the curves, you will see that it is practically not necessary to enter upon any calculation for spherical aberration. We know about what form gives an approximate correction; we adhere nearly to that, and the rest is done by figuring of the surface.

To illustrate what I mean. I would be quite willing to undertake to alter the curves of the crown or flint lens of any of my objectives by a very large quantity, increasing one and decreasing the other so as to still satisfy the conditions of achromatism, but introducing theoretically a large amount of positive or negative spherical aberration, and yet to make out of the altered lens an object-glass perfectly corrected for spherical aberration.

I am now speaking of ordinary sizes. For very large sizes it is usual to go more closely into the calculations; but I may remark that it is sometimes possible to make a better objective by deviating from the curves which give a true correction for spherical aberration and correcting that aberration by figuring, rather than if the strictly theoretical curves were adhered to. So far, then, as any calculation is required, the ordinary formulæ given in the text-books may be considered amply sufficient.

Having now determined on the curves, we have to consider the various processes which the glass has to undergo from the time it is received in this form from the glass manufacturer to the time when it is turned out a finished objective.

The work divides itself into five distinct operations: (1) rough grinding; (2) fine grinding; (3) polishing; (4) centering; (5) figuring and testing.

(1) The rough grinding or approximate shaping of the glass is a very simple process. The glass is cemented on a holder, and is held against a revolving tool supplied with sand and water, and of a shape which will tend to abrade whatever portions are necessary to be removed to produce the required curves. These diagrams will illustrate the various operations.

(2) Fine grinding. The tools used for fine grinding are of this form, and are made of either brass or cast iron. I prefer cast iron, except for very small sizes. They are grooved on the face, in the manner suggested by the late Mr. A. Ross, in order to allow the grinding material to properly distribute itself.

If two spherical surfaces be rubbed together they will, as may be supposed, tend to keep spherical; for the spherical is the only curve which is the same radius every part of its surface. If fine dry abrading powder be used between, the same result will be obtained; but, if wet powder be used, the surface will no longer continue spherical, but will abrade away more on the centre and edge than in the zone between. It was to meet this difficulty that the late Mr. A. Ross devised the idea of the distributing grooves. The fine grinding process is the first of the series which calls for any skill on the part of the operator.

That the *modus operandi* of the grinding be the more easily understood, let me explain the principle of the process in a few words.

When two surfaces of unequal hardness are rubbed together with emery powder and water between the two, each little particle of the powder is at any given moment in either of these conditions: (*a*) embedded into the softer surface; (*b*) rolling between the two surfaces; (*c*) sliding between the two surfaces.

Those particles which become embedded in the softer material do no work in abrading it, and but little in abrading the harder. They generally consist of the finer particles, and are kept out of action by the coarser which are rolling or sliding between the surfaces. Further, those that are purely rolling do little or no work. The greater part of the work is performed by those particles which are facetted and which *slide* between the two surfaces.

As the grinder is always of a much softer material than the glass, there is much more friction between the grinder and these particles than between the glass and the same particles, and therefore they partially adhere to the grinder and are carried by it across the face of the glass. This being so, it is now easy to perceive what the best conditions for rapid grinding are. Not too little emery, for then there will not be enough of abrading particles; not too much, for then the particles will roll on each other and tend to crush and disintegrate each other instead of abrading the glass, but just sufficient to form a single layer of particles between the grinder and the glass surface.

In the grinding of the small lenses, I mean up to 5 or 6 inches diameter, it is usual to carry out the entire grinding processes by hand; above that size by machinery. Surfaces up to 12 or even 15 inches can be ground by hand; but the labour becomes severe, and for my part I am gradually reducing the size for which the hand grinding is used, as I find the machine work more constant in its effects.

The machinery used is the same as that employed for the polishing operation, and I shall describe it under that head further on.

In the fine grinding operation by hand, the glass is usually cemented on to a holder of this form, having (for smaller sizes) three pieces of cork, to which the lens is attached, and this holder being screwed to a spud or nose on top of a post screwed to the floor. The operator, having applied the proper quantity of moist emery powder between the grinder and the glass, proceeds to work the former over the latter in a set of peculiar strokes, the amplitude and character of which he varies according to circumstances, at the same time that he changes his position round the post every few seconds. . . .

Although, as I have shown, the harder material is abraded very much more than the softer, yet the softer (the grinder) suffers considerable abrasion as well as the glass, and the skill of the operator is shown by the facility with which he is able to bring the glass to the curve of the grinder without altering the curve or figure of the latter.

It is even possible for a skilled operator to take a lens of one curve and a grinder of, say, a deeper curve, and by manipulation to produce a pair of surfaces fitting together, and of shallower curves than either.

Measurement of the Curves.—In the early stages of grinding, gauges of the proper radius, cut out of sheet brass or sheet steel, are used for roughly testing the curves of the lenses; but when we get to the finer grinding process it is necessary to have something much more accurate.

For this purpose a spherometer is used. It is made in various forms, generally with three legs terminating in three hardened steel points, which lie on the glass, and a central screw with fine thread, the point of which can be brought down to bear on the centre of the glass. In this way the versed sine of the curve for a chord equal to diameter of circle formed by these points is measured, and the radius of curve can be easily calculated from this.

I do not find the points satisfactory for regular work. They are apt to get injured or worn, and for ground surfaces are a little uncertain, as one or other of the feet may find its way into a deep pit. This particular spherometer has three feet, of about half an inch long, which are hardened steel knife-edges forming three portions of an entire circle. In using this it is laid on the surface to be measured, and the screw with micrometer head is turned till the point is felt to touch the surface of glass. This scale and head can then be read off. The screw in this instrument has fifty threads to the inch, and the head is divided into 100 parts, so that each division is equal to $\frac{1}{5000}$ of an inch. With a little practice it is easy to get determinate measures to $\frac{1}{10}$ of this, or $\frac{1}{50000}$ of an inch, and by adopting special precautions even more delicate measures can be taken, as far probably as $\frac{1}{100000}$ or $\frac{1}{150000}$ of an inch, which I have found to be practically the limit of accuracy of mechanical contact.

To give an idea of the delicacy of the instrument, I bring the screw firstly into contact with the glass. Now the screw is in good contact; but there is so much weight still on the three feet, that, if I attempt to turn it round, the friction on the feet oppose me, and it will not stir except I apply such force as will cause the whole instrument to slide bodily on the glass. Now, however, I raise the whole instrument, taking care that my hands touch none of the metal-work, and that the screw be not disturbed. I lay my hands for a moment on part of the glass where centre screw stood, and thus raise its temperature slightly, and on laying the spherometer back in the same place, you now see that it spins on the centre screw, showing how easily it detects what to it is a large lump, caused by expansion of the glass from the momentary contact of my hand.

Flexure.—One of the greatest difficulties to be contended with in the polishing of large lenses is that of flexure during the process.

It may appear strange that in disks of glass of such considerable thickness as are used for objectives, any such difficulty should occur; but a simple experiment will demonstrate the ease with which such pieces of glass can be bent, even under such slight strain as their own weight.

We again take our spherometer and set it upon a polished surface of a disk of glass of about 7½ inches diameter and ¾ inch thick. I set the micrometer head as in the former experiment to bear on the glass, but not sufficiently tight to allow the instrument to spin round. This has now been done while the glass, as you see, is supported on three blocks near its periphery. I now place one block under the centre of disk and remove the others thus, and you see the instrument now spins round on centre screw.

It is thus evident that not only is this strong plate of glass bending under its own weight, but it is bending a quantity easily measurable by this instrument, which, as I shall presently show, is quite too coarse to measure such quantities as we have to deal with in figuring objectives.

After this experiment no surprise will be felt when I say that it is necessary to take very special precautions in the supporting of disks during the process of polishing, to prevent danger of flexure; of course if the disks are polished while in a state of flexure, the resulting surface will not be true when the cause of flexure is removed.

For small-sized lenses no very special precautions are necessary, but for all sizes over 4 inches in diameter I use the equilibrated levers devised by my father, and utilised for the first time on a large scale in supporting the 6-foot mirror of Lord Rosse's telescope. These have been elsewhere frequently described, but I have a small set here as an example.

I have also sometimes polished lenses while floating on mercury. This gives a very beautiful support, but it is not so convenient, as it is difficult to keep the disk sufficiently steady while the polishing operation is in progress without introducing other chances of strain.

So far I have spoken of strain or flexure during the process of working the surface; but even if the surface be finished absolutely perfectly, it is evident from the experiment I showed you that very large lenses when placed in their cells must suffer considerable flexure from their own weight alone, as they cannot then be supported anywhere except round the edge.

To meet this I proposed many years ago to have the means of hermetically sealing the tube, and introducing air at slight pressure to form an elastic support for the objective, the pressure to be regulated by an automatic arrangement according to the altitude. My attention was directed to this matter very pointedly a few years ago from being obliged to use for the Vienna 27-inch objective a crown lens which was, according to ordinary rules, much too thin.

I had waited some years for this disk, and none thicker could be obtained at the time. This disk was very pure and homogeneous, but so thin that, if offered to me in the first instance, I would certainly have rejected it. Great care was taken to avoid flexure in the working, but, to my great surprise, I found no difficulty whatever with it in this respect. This led me to investigate the matter, with the following curious results.

If we call f the flexure for any given thickness t, and f' the flexure for any other thickness t', then $\frac{f}{f'} = \frac{t^2}{t'^2}$ for any given load or weight approximately. But as the weight increases directly as the thickness, the flexure of the disks due to their own weight, which is what we want to know, may be expressed as $\frac{f}{f'} = \frac{t}{t'}$.

Let us now consider the effect of this flexure on the image. In any lens bent by its own weight, whatever part of its surface is made more or less convex or concave by the bending has a corresponding part bent in the opposite direction on the other surface, which tends to correct the error produced by the first surface. This is one reason why reflectors which have not this second correcting surface are so much more liable to show strain than refractors. If the lens were infinitely thin, moderate flexure would have no effect on the image. The effect increases directly as the thickness. If then the flexure, as I have shown, decreases directly as the thickness, and the effect of that flexure increases directly as the thickness, it is clear that the effect of flexure of any lens due to its own weight will be the same for all thicknesses; in other words no advantage is gained by additional thickness.

This has reference, of course, only to flexure of the lens in its cell after it has been duly perfected, and has nothing to do with the extra difficulty of supporting a thin lens during the grinding and polishing processes.

Polishing.—The polishing process can be, and is often, conducted precisely in the same manner as the grinding, except that the abrading powders used (oxide of iron, rouge, an oxide of tin, putty-powder) is of a finer and softer description, and the surface of the polishing tool is made of a softer material than the metallic grinder.

Very nearly all my polishing is done on the machine I shall presently describe; but before doing so, I will, with your permission, say a few words on the general principles of the polishing process. Various substances are used for the face of the polisher—fine cloth, satin or paper, and pitch. Pitch possesses two important qualities which render it peculiarly suitable for this work, and it is a curious fact that we owe its application for this purpose to the extraordinary perspicuity of Sir Isaac Newton, who we may fairly say was the first to produce an optically perfect surface, and that that material is not only still used for the purpose, but is, as far as I know, the only substance which possesses the peculiar qualifications necessary to fulfil the required conditions. With skill and care, moderately good surfaces can be obtained from cloth polishers; but it is easy to see why they can never be perfect. There is a certain amount of elasticity in cloth and in its "nap," and there is consequently a tendency to round off the surfaces of the pits left by the grinding powder, and to polish the bottom or floor of these pits at the same time as the upper surface. It is easy to show mathematically that any process which abrades the floors of the pits at the same time as general surfaces even in a very much less degree, can never produce more than an approximation to a perfect surface, and practice agrees with the theory. Paper is said to be much used by the French opticians. I can say nothing about it. I have tried it and failed to produce a perfect surface with it, nor indeed should I expect it. It is of course open to the same objection as cloth. Pitch possesses, as I said, two most important qualities which render it suitable for the work; the first, in its almost perfect inelasticity; the second, a curious quality of subsidence, which we utilise in the process.

If we watch with a microscope, or even a magnifier, the character of two surfaces during the process of polishing, the one with cloth, and the other with pitch, the difference is very striking. With the cloth polisher, the polish appears much quicker, and it would at first sight appear as if the same polishing powder abraded more quickly on the cloth than on the pitch polisher, but I do not believe that such is the case, for if we look at the surface with a magnifier we shall find that, while all the surface has assumed a polished appearance, the surface itself has retained some of the form of the original pitted character with the edges rounded off; while in the pitch half-polished surfaces the floors of the pits are as gray as ever, and the edges are sharp and decisive. In pitch polishing, too, a decided amount of polish appears very quickly, and then for many hours there appears to be little or no further effect. Suddenly, however, the remaining grayness disappears, and the surface is polished. The reason of this is very obvious. The polisher being very inelastic, polishes first only the tops of the hills, and has to abrade away all the material of which these hills are composed before it reaches the valleys or floors of the pits. When it does reach them, the proper polish quickly appears. The second quality of pitch, that of subsidence, is also most valuable.

Pitch can be rendered very hard by continued boiling. By pitch I mean the natural bituminous deposit which comes to us from Archangel, not gas-tar pitch. It can be made so hard that it is impossible to make any impression on it with the finger-nail without splitting it into pieces; and yet even in this hard condition, if laid on an uneven surface it will in a few days, weeks, or months subside and take the form of whatever it is resting upon. The cohesion of its particles is not sufficient to enable it to retain its form under the action of gravity; and as this condition is that which science tells us marks the difference between solids and liquids, we must, paradoxical though it may appear, class even the hardest pitch among liquid instead of solid substances.

Now how do we utilise this peculiar quality?

The polishing tool is made by overlaying a metal or wooden disk formed to nearly the required curves by a set of squares of pitch, and while these are still warm pressing them against the glass, the form of which they immediately take.

In the grinding process I showed you that the regulation of the abrasion was managed partly by the character of the stroke given, and partly by the local touches given to the tool by the stoning process.

In polishing we still retain the same facilities for modifying the stroke, and the same rules I gave apply generally to the polishing process as well as the grinding; but we have not got any process equivalent to that of the local stoning, and even if we had it would be useless, for this very quality of subsidence of the pitch would in a few minutes cause any part of its surface which had been reduced to come into good contact again; we must therefore look for some other means for producing more or less abrasion whenever we require it. This we effect by modifying the size of the squares of pitch in the various zones. Practically it is done in this way by a knife and mallet. Whenever the squares are reduced, the abrasion will be less.

This is a well-known method of regulation; but the rationale is, I think, not generally understood. It is generally explained that there is less abrasion because there is less abrading surface. I do not think this is the true, or at least the entire, explanation. In order to understand the action, you must conceive the pitch to be constantly in a state of subsidence, the amount of that subsidence depending of course on the pressure placed upon it. Now, if we reduce the size of the squares in any zone while retaining the same distance from centre to centre of squares, we increase at first the pressure per unit of area on the pitch squares in that zone, and consequently the subsidence will be greater, and the action will not be so tight or severe on that zone.

I know of no substance but pitch and a few of the resins which possesses this peculiar quality except perhaps ice, and it is curious to think that the same quality which in ice allows of that gradual creeping and subsidence and consequent formation of glaciers with their characteristic moraines, &c., will in pitch help us to produce accurate optical surfaces.

Polishing-Machines.—The two best-known polishing-machines are those of the late Earl of Rosse and the late Mr. Lassell, the general forms of which were shown in these diagrams. Time will not permit me to enter into a minute description of their working, nor is it necessary, as both have been often described.

A few words, however, as to the different character of the strokes given by these machines may be interesting. The stroke of Lord Rosse's machine may be imitated in hand-work by the operator traversing the polisher or mirror in a series of nearly straight strokes, of about one-third the diameter of the glass, to and from himself, at the same time that he keeps walking slowly round the post, and instead of allowing the centre of polisher to pass directly over the centre of mirror, each stroke that he gives he slides a little (about one-tenth diameter) to one side and then a little to the other.

Mr. Lassell's stroke may be imitated by causing the polisher to describe a series of nearly circular strokes a little out of the centre, walking round the post at the same time; thus the centre of polisher will describe a series of epicycloidal or hypocycloidal curves on the speculum.

Many years ago my father devised a machine, figured and described in Nichols's "Physical Science," by which either of these motions could be obtained. He appeared to have got better results with Mr. Lassell's strokes, for he afterwards devised a machine which gave the same character of stroke, but over which the operator had greater control, and this machine has been used for many years with great success. Like all machines, however, which give a series of strokes constantly recurring of the same amplitude, it is apt to polish in rings It is impossible to obtain absolute homogeneity in the pitch patches, and if any one square be a shade harder than the general number, and that square ends its journey at each stroke at the same distance from the centre of speculum or glass, there will almost surely be a change of curvature in that zone. To avoid this I have made a slight modification in the machine, which has increased its efficiency to a great extent. I will now place in the lantern a model of this machine, and first draw you a few curves with the machine in its old state, and afterwards in its improved state.

In order to convey some idea of the relative quantities of material removed by the various processes, I have placed upon the walls a diagram which will illustrate this point in two distinct ways.

The diagram itself represents a section of a lens of about 8 inches aperture and 1 inch thick, magnified 100 times, and shows the relative thickness of material abraded by the four processes.

The quantity removed by the rough grinding process is represented on this diagram by a band 25 inches wide, the fine grinding by one $\frac{8}{10}$ inch wide, the polishing by a line $\frac{1}{50}$ inch wide, while the quantity removed by the figuring process cannot be shown even on this scale, as it would be represented by a line only $\frac{1}{10000}$ inch thick.

I have also marked on this diagram the approximate cost of abrasion of a gramme of material by each of the four processes, viz.:—

		£	s.	d.	
Rough grinding,	about	0	0	1	per gramme.
Fine grinding,	,,	0	0	7½	,,
Polishing,	,,	0	10	0	,,
Figuring,	,,	48	0	0	,,

Figuring and Testing.—By the figuring process I mean the process of correcting local errors in the surfaces, and the bringing of the surfaces to that form, whatever it may be, which will cause the rays falling on any part to be refracted in the right direction. When an objective has undergone all the processes I have described, and many more which are not so important, and with which I have not had time to deal, and when the objective is centred and placed in its cell, it is, to look at, as perfect as it will ever be, but to look through and use as an objective it may be useless. The fact is that when an objective has gone through all the processes described, and is in appearance a finished instrument, I look upon it as about one-fourth finished. Three-fourths of the work has probably to be done yet. True, sometimes this is by no means the case, and I have had instances of objectives which were perfect on the first trial; but this is, I am sorry to say, the exception and not the rule.

This part of the process naturally divides itself into two distinct heads:—

(1) The detection and localisation of faults—what may, in fact, be termed the diagnosis of the objective.

(2) The altering of the figures of the different surfaces to cure these faults. This may be called the remedial part.

It may be well here to try to convey some idea of the quantities we have to deal with, otherwise I may be misunderstood in talking of great and small errors.

I have before mentioned that it is possible to measure with the spherometer quantities not exceeding $\frac{1}{50000}$ of an inch, or with special precaution much less even than that; but useful as this instrument is for giving us information as to the general curves of the surface, it is utterly useless in the figuring process; that is, an error which would be beyond the power of the spherometer to detect, would make all the difference between a good and a bad objective.

Take actual numbers and this will be evident. Take the case of a 27-inch objective of 34 feet focus; say there is an error in centre of one surface of about 6 inches diameter, which causes the focus of that part to be $\frac{1}{10}$ of an inch shorter than the rest.

For simplicity's sake, say that its surface is generally flat; the centre 6 inches of the surface therefore, instead of being flat, must be convex and of over 1,000,000 inches radius. The versed sine of this curve, as measured by spherometer, would be only about $\frac{1}{250000}$, 4 millionths of an inch, a quantity mechanically unmeasurable, in my opinion.

If that error was spread over 3 inches only instead of 6 inches, the versed sine would only be about $\frac{1}{1000000}$. Probably the effect on the image of this 3-inch portion of $\frac{1}{10}$ inch shorter focus would not be appreciable on account of the slight vergency of the rays, but a similar error near edge of objective certainly would be appreciable. Until, therefore, some means be devised of measuring with certainty quantities of 1 millionth of an inch and less, it is useless to hope for any help from mechanical measurement in this part of the process.

If, then, no known mechanical arrangement be delicate enough to measure these quantities, how, it may be asked, are these errors detected?

The answer to this is, that certain optical arrangements enable us to carry our investigations far beyond the limits of mechanical accuracy. Trials of the objective or mirror as a telescope are really the crucial test, but there are various devices by which defects are detected and localised.

The best object to employ is generally a star of the third or fourth magnitude, when such is available, but as it frequently occurs that no such object is visible, recourse is had to artificial objects. The minute image of the sun reflected from little polished balls of speculum metal, or even a thermometer bulb is a very good object; polished balls of black glass are also used with good effect; but as the sun also is of somewhat fickle disposition in this country, we have frequently to have recourse to artificial light. Small electric lamps, such as this, with their light condensed and thrown on a polished ball are very useful. In fact, I am never without one of them in working order.

For the detection and localisation of errors it is very useful to be provided with sets of diaphragms which leave exposed various zones of the surface, the foci of which can then be separately measured, but a really experienced eye does not need them.

For concave surfaces, Foucault's test is useful. I shall not trespass on your time to explain this in detail, as it is described very fully in many works, in none better than in Dr. Draper's account of the working of his own reflecting telescope. This diagram will give an idea of the principle of the system, which is really the same as what I have described as useful for detecting want of homogeneity in the substance of the glass.

This system is extremely useful for concave spherical surfaces, but is not available for convex surfaces, and only partially available for concave parabolic surfaces.

The really crucial test is, as I said before, the performance of the objective when used as a telescope; and the appearance of the image not only at the focus, but on each side of it, conveys to the practised eye all the information required for the detection of the errors.

If an objective have but one single fault, its detection is easy; but it generally happens that there are many faults superposed, so to speak. There may be faults of achromatism, and faults of figure in one or all the surfaces; faults of adjustment, and perhaps want of symmetry from some strain or flexure; and the skill of the artist is often severely taxed to distinguish one fault from the rest and localise it properly, particularly if, as is generally the case, there be also disturbances in the atmosphere itself, which mask the faults in the objective, and permit of their detection only by long and weary watching for favourable moments of observation.

It would be impossible in one or a dozen of such lectures as this to enumerate all the various devices that are practised for the localisation of errors, but a few may be mentioned, some of which have never before been made public.

For detection of faults of symmetry, it is usual to revolve one lens on another and watch the image. In this way it can generally be ascertained whether it is in the flint or crown lens.

With some kinds of glass the curves necessary for satisfying the conditions of achromatism and spherical aberration are such that the crown becomes an equi-convex and the flint a nearly plano-concave of same radius on inside curve as either side of the crown. This form is a most convenient one for the localisation of surface errors in this manner.

The lenses are first placed in juxtaposition and tested. Certain faults of figure are detected. Now calling the surfaces A B C D in the order in which the rays pass through them, place them again together with Canada balsam or castor-oil between the surfaces B and C, forming what is called a cemented objective. If the fault be in either A or D surface, no improvement is seen; if in B or C, the fault will be much reduced or modified. Now reverse the crown lens, cementing surfaces A and C together. If same fault still shows, it must be in either B or D. If it does not show, it will be in either A or C. From these two experiments the fault can be localised.

It often happens that a slight error is suspected, but its amount is so slight that it appears problematical whether an alteration would really improve matters or not. Or the observer may not be able to make up his mind as to the exact position of the zone he suspects to be too high or too low, and he fears to go to work and perhaps do harm to an objective on which he has spent months of labour, and which is almost perfect. In many such cases I have wished for some means by which I could temporarily alter the surface and see it so altered before actually proceeding to abrade and perhaps spoil it.

During my trials with the great objective of Vienna, I thought of a very simple expedient, which effects this without any chance at all of injuring the surface. If I suspect a certain zone of an objective is too low, and that the surface might be improved by lowering the rest of it, I simply pass my hand, which is always warmer than the glass, some six or eight times round that particular zone. The effect of this in raising the surface is immediately apparent, and is generally too much at first, but the observer at the eye end can then quietly watch the image as the effect goes off, and very often most useful information is thus obtained. The reverse operation, that of lowering any required part of the surface, is equally simple. I take a bottle of sulphuric ether and a camel's-hair brush, and pass the brush two or three times round the part to be lowered, blowing on it slightly at the same time; the effect is immediately perceived, and can always be overdone if required.

So far then for the diagnosis. Now for the remedy. When the fault has been localised, the lens is again put upon the machine and the polisher applied as before, the stroke of the machine and the size of the pitch patches being so arranged as to produce, or tend to produce, a slightly greater action on those parts that have been found to be too high (as before described while treating of the polishing processes).

The regulation of the stroke, excentricity, speed, and general action of the machine, as well as the size and proportion of the pitch squares, and the duration of the period during which the action is to be continued, are all matters the correct determination of which depends upon the skill and experience of the operator, and concerning which it would be impossible to formulate any very definite rules. All thanks are due to the late Lord Rosse and Mr. Lassell, and also to Dr. de la Rue, for having published all particulars of the process which they found capable of communication; but it is a notable fact that, as far as it is possible to ascertain, every one who has succeeded in this line has done so, not by following written or communicated instructions, but by striking out a new line for himself; and I think I am correct in saying that there is hardly to be found any case of a person attaining notable success in the art of figuring optical surfaces by rigidly following directions or instructions given or bequeathed by others.

There is one process of figuring which is said to be used with success among Continental workers. I refer to the method called the process of local touch. In this process those parts, and those parts only, which are found to be high, are acted upon by a small polisher.

This action is of course much more severe; and if only it were possible to know exactly what was required, it ought to be much quicker; but I have found it a very dangerous process. I have sometimes succeeded in removing a large lump or ring in this way (by large I mean 3 or 4 millionths of an inch), but I have also and much oftener succeeded in spoiling a surface by its use. I look upon the method of local touch as useful in removing gross quantities, but for the final perfecting of the surface I would not think of employing it.

In small-sized objectives the remedial process is the most troublesome, but in large-sized objectives the diagnosis becomes much the more difficult, partly on account of the rare occurrence of a sufficiently steady atmosphere. In working at the Vienna objective it often happened when the figure was nearly perfect that it was dangerous to carry on the polishing process for more than ten minutes between each trial, and we had then sometimes a week to wait before the atmosphere was steady enough to allow of an observation sufficiently critical to determine whether that ten minutes' working had done harm or good. It must not be supposed either that the process is one in which improvement follows improvement step by step till all is finished. On the contrary, sometimes everything goes well for two or three weeks, and then from some unknown cause, a hard patch of pitch perhaps or sudden change of temperature, everything goes wrong. At each step, instead of improvement there is disimprovement, and in a few days the work of weeks or months perhaps is all undone. Truly any one who attempts to figure an objective requires to have the gift of patience highly developed.

In view of the extraordinary difficulty in the diagnostic part of the process with large objectives, it is my intention to make provision which I hope may reduce the trouble in the working of the new 28-inch objective for the Royal Observatory, Greenwich.

Two of the greatest difficulties we have to contend with are: (1) the want of homogeneity in the atmosphere, through which we have to look in trials of the objective, due to varying hygrometric and thermometric states of various portions; and (2) sudden changes of temperature in the polishing-room. The polisher must always be made of a hardness corresponding to the existing temperature. It takes about a day to form a polisher of large size, and if before the next day the temperature changes 10° or 15°, as it often does, that polisher is useless, and a new one has to be made, and perhaps before it is completed another change of temperature occurs. To grapple with these two difficulties I propose to have the polishing-chamber under ground, and, leading from it, a long tunnel formed of highly glazed sewer-pipes about 350 feet long, at the end of which is placed an artificial star illuminated by electric light; on the other side of the polishing-chamber is a shorter tunnel, forming the tube of the telescope, terminating in a small chamber for eye-pieces and observer. About half-way in the long tunnel there will be a branch pipe connected to the air-shaft of the fan, which is used regularly for blowing the blacksmith's fire, and through this, when desired, a current of air can be sent to "wash it out" and mix up all currents of varying temperature and density. It may be found necessary even to keep this going during observations.

By this arrangement I hope to be able to have trials whenever required, instead of having to wait hours and days for a favourable moment.

Figuring of Plane Mirrors.—There is a general idea that the working of a plane mirror or one of very long radius is a more difficult operation than those of more ordinary radii. This is not exactly the case. There is no greater difficulty in figuring a low curve than a deep one, but the difficulty in the case of absolutely plane mirrors consists simply in the fact that in their figuring there is one additional condition to be fulfilled, viz. that the general radius of curvature must be made accurate within very narrow limits. In figuring a plane mirror to use, for instance, in front of even a small objective, say 4-inch aperture, an error in radius which would cause a difference of focus of $\frac{1}{100}$ of an inch would seriously injure the performance. This would be about equivalent to saying that the radius of curvature of the mirror was about 8 miles, the versed sine of which, with the 6-inch spherometer, would be about $\frac{1}{500000}$ of an inch. Now what I mean to convey is this: that it would be just as difficult to figure a convex or concave lens of moderate curvature as a flat lens of the same size if it were necessary to keep the radius accurate to that same limit, *i.e.* one-tenth of a division of this spherometer.

Lick Observatory.—For the final testing of large objectives or mirrors, it is necessary to have them properly mounted, and in such a manner that they can be directed conveniently on any celestial object, and kept so directed by clockwork, to enable the observer to devote his whole attention to the testing. I had not intended touching at all on the subject of the mounting of telescopes, but I have been asked to call your attention to this model of a proposed observatory for Mount Hamilton, California, as it embraces some novel features, but I shall do so in only a very few words.

Most here are probably aware that a monster observatory is in course of erection in California, a large sum of money having been left for the purpose by a Mr. Lick. The observatory is already partly complete, and contains some excellent instruments of moderate size, the work already done with which warrants us to hope that the great 36-inch refractive about to be erected will be placed under more favourable conditions for work than any other large telescope in the world.

The 36-inch objective is at present in process of construction by the Messrs. Clark of America, but the mounting has not yet been contracted for.

Some years since, in a paper published in the *Transactions* of the Royal Dublin Society, I shadowed forth a principle which I considered should be adopted in great telescopes of the future. The trustees of the Lick Observatory having invited me to design an instrument for the 36-inch objective, I have put into practical form what I had before given but general principles of, and the design which this model illustrates is the result.

Whether this design will ever be carried out or not I cannot tell, but even as a proposal I trust it may be interesting enough to excuse my introducing it (somewhat irrelevantly perhaps) to your notice to-night.

The design includes the equatorial itself, with its observatory, dome, and provision for enabling the observer to reach the eye end conveniently.

The conditions I laid down for myself in designing this observatory were that it would be possible for the observer single-handed to enter the equatorial room at any time, and that, without using more physical exertion than is necessary for working the smallest-sized telescope, or even a table-microscope, he should be able to open the 70-foot dome, turn it round backwards and forwards, point the equatorial to any part of the heavens, revolving it in right ascension and declination to any extent, and finally (the most difficult of all) to bring his own person into a convenient position for observing. I say this last is the most difficult of all, for I think any who have worked with larger instruments will allow that there is generally far more trouble in moving the observatory chair (so called) and placing it in proper position than in pointing the instrument itself. In this instrument the "chair" would require to be 25 feet high, and with its movable platform, ladder, balance-weight, &c., would weigh probably some tons. Even if very perfect arrangements were made for the working of this chair, the mere fact that the observer, while attempting to make the most delicate observations, is perched upon a small and very unprotected platform 25 feet above the floor, and in perfect darkness, tends to reduce his value as an observer to an extent only to be appreciated by those who have tried it.

No matter how enthusiastic a man may be at his work, I would not put a high value on his determinations if made while in a position which calls for constant anxiety for his own personal safety. I would go even further still, and say that even personal comforts or discomforts have much to do with the value of observations.

I propose, therefore, that all the various motions should be effected by water-power. There are water engines of various forms now made, some of which have no dead point, and having little *vis inertia*, are easily stopped and started, and are consequently well adapted for this work.

I propose to use four of them: one for the right ascension motion of the instrument, and one for the declination; one for revolving the dome, and one for raising and lowering the observer himself; but instead of having anything of the nature of a 25-foot chair or scaffold, I propose to make the 70-foot floor of the observatory movable. It is balanced by counterpoise weights, and raised and lowered at will by the observer. Then the observer can without any effort raise and lower the whole floor, carrying himself and twenty people if desired, to whatever height is most convenient for observation; and wherever he is observing, he is conscious that he has a 70-foot floor to walk about on, which even in perfect darkness he can do in safety.

The valves and reversing gear of the water engines are actuated by a piece of mechanism, the motive power of which may be a heavy weight raised into position some time during the previous day by man- or water-power. By means of a simple electrical contrivance, this piece of machinery itself is under the complete control of the observer, in whatever part of the room he may be, and he carries with him a commutator of a compact and convenient form, with eight keys in four pairs, each pair giving forward and backward movements respectively to (A) telescope movement in right ascension; (B) telescope movement in declination; (C) revolution of dome; (D) raising of floor.

The remaining operation—opening of shutter—is easily effected without any additional complication.

It is only necessary to anchor the shutter (which moves back horizontally) to a hook in the wall and move the dome in the opposite direction by motion C; the shutter must, of course, be opened by this motion. . . .

It is very possible that there may be some here who have found what I have had to say on the subject of the figuring of objectives very unsatisfactory. They may have expected, naturally enough, that, instead of treating of generalities to such a large extent as I have done, I should have given precise directions, by the following of which rigidly any person could make a telescopic objective.

To those, however, who have followed me in my remarks, the answer to this will probably have already suggested itself. It is the same answer which I give to those who visit my works and ask what the secrets of the process are, or if I am not afraid that visitors will pick up my secrets. All the various processes which I have described up to that of the figuring are, I consider, purely mechanical processes, the various details of which can be communicated or described as any mechanical process can be; but in the last final and most important process of all there is something more than this. A person might spend a year or two in optical works where large objectives are made, and might watch narrowly every action that was taken, see every part of the process, take notes, and so forth, and yet he could no more expect to figure an objective himself than a person could expect to be able to paint a picture because he had been sitting in an artist's studio for the same time watching him at his work. Experience, and experience only, can teach any one the art, and even then it is only some persons who seem to possess the power of acquiring it.

A well-known and experienced amateur in this work declared his conviction that no one could learn the process under nine years' hard work, and I am inclined to think his estimate was not an exaggerated one.

True, it may be said that large objectives can be and are generally turned out by machinery, but what kind of an objective would any machine turn out if left to guide itself, or left to inexperienced hands?

At the risk of being accused of working by what is generally called the rule of thumb, I confess that conditions often arise, to meet which I seem to know intuitively what ought to be done, what crank to lengthen, what tempering is required of the pitch square; and yet if I were asked I should find it very hard to give a reason for my so doing which would even satisfy myself.

I may safely say that I have never finished any objective over 10 inches diameter, in the working of which I did not meet with some new experience, some new set of conditions which I had not met with before, and which had then to be met by special and newly-devised arrangements.

A well-known English astronomer once told me that he considered a large objective, when finished, as much a work of art as a fine painting.

I have myself always looked upon it less as a mechanical operation than a work of art. It is, moreover, an art most difficult to communicate. It is only to be acquired by some persons, and that after years of toilsome effort, and even the most experienced find it impossible to reduce their method to any fixed rules or formulæ.

INDIAN CASTINGS AT THE INDIAN AND COLONIAL EXHIBITION

AT the last meeting of the Iron and Steel Institute Mr. C. Purdon Clarke, C.I.E., Keeper of the Indian Section, South Kensington Museum, read a paper "On Certain Descriptions of Indian Castings" as follows :—

The importation of partly manufactured material is at present exercising considerable influence over many of the native arts of Oriental countries and India. The supply of machine-made thread has doubled the village hand-looms in some districts of Madras, and gold thread from Germany has enabled the brocade weavers to compete with the imitation brocades sent in from Europe.

In some handicrafts, however, the supply of European material has produced a contrary effect. Iron and steel, bar and rod, have displaced an ancient industry, and sheet copper and brass have robbed the founder of half his work. Formerly the only means of producing sheet-metal was by hammering cast plates, an expensive method, only resorted to when thin flat coverings were required for wooden or other objects. For very large vessels, where weight was required to be kept down and strength maintained, hammered sheet was used ; but generally the founder was employed, to save as much as possible the labour of forming the furnished castings which required but little beating out, trimming, and brazing.

In the case of a bowl, or flat jar with a narrow mouth, the founder would prepare a cast not unlike in shape and thickness that of an ordinary flower-pot saucer, from which, by constant hammering, the bulbous sides would be formed, projecting beyond the rim, which would remain of its first diameter and thickness. When finished, such a vessel would be nearly double the size of the first cast, and a remarkable example of the native knowledge of the composition of bronzes and annealing processes.

It is worthy of noting that the chief means of detecting modern from old Persian and Saracenic metal vessels is by examining the brazen joints, which in ancient vessels are rare. When not found, a close examination will show the vessel to be a thin casting, the ornamentation being by inlay, or chasing and hammering, which, being done after the cast is made, gives the reverse side the appearance of chased sheet metal.

So far as he could ascertain, there were three methods of casting practised in India. The first, by moulds in sand ; the second, moulds in clay not unlike plasterers' piece-moulds ; the third, clay moulds formed on a wax model, the *cire perdu* of Europe.

The first of these was well known in Europe, but the second was, he believed, now described for the first time. In preparing the mould, impressions of the various parts of the pattern are taken in clay, and these pieces when nearly dry are, after trimming, stuck neatly together, and kept in place by several layers of mud, in which some fibre is mixed. The mould when ready has but one vent, which, placed on the most convenient side, is carried up into a sort of bottle-neck. If the object is small, several moulds are attached together, and the vents united by a single short neck of clay, to which a crucible, inclosed in an egg-shaped ball of clay, is attached. The size of this crucible depends upon the exact amount of metal required to fill the mould or moulds ; and this quantity being known by experience, the founder places it inside before closing up. No provision is made for the escape of air from the mould when the metal is poured in. The mould and crucible (now in one piece) is allowed to dry ; and after several coats of clay, tempered with fibre, have also been well baked on by the sun, the furnace is prepared. This is simply a circular chamber about 2 feet 6 inches in diameter, 2 feet in height, with a perforated hearth and no chimney. Half filled with charcoal, a good heat is obtained by the use of several sheepskin bellows from beneath. When ready, as many moulds as the furnace will hold are placed in it, the crucible end of each being embedded in the fire. A cover is placed over, and the fire kept up until, upon examination, the moulds are found to be red hot. They are then taken, one at a time, and replaced in a reverse position, the crucibles being now above. The metal flows down into a red-hot mould' and penetrates the finest portions of the surface without suffering from air or chilling. The fire is allowed to gradually cool, and when the objects are broken out of their clay covering, the metal is soft and malleable.

The third manner of casting (that by the use of a wax pattern which is destroyed in the moulding) was well known, but in one particular case the process had been carried further than would be at first believed, and of this he would now attempt a description.

The object produced is an anklet, a flexible ring about 4 inches in diameter, made from an endless curb chain. Such curb chain trinkets are common in India, and are generally made from thick silver wire rings interlinked and soldered one by one. In this example the anklet is of bronze, and consists of a complicated chain of forty-three detailed links, the whole being cast by a single operation. The first part of the process is the preparation of a pattern in wax, a delicate work, each link having to pass through four others, and to bear three small knobs or rosettes. These are in two instances but ornaments ; the third, however, serves as a channel for the metal to enter each ring.

Then commences the most difficult part of the work, each ring having to be slightly separated, and this is effected by painting in a thin coat of fine clay until there is sufficient to form a partition. Other coats of clay are added until a thickness of about half an inch is attained, when a groove is cut round the upper side of the ring, and deepened until the row of knobs is bared. The wax is then melted out, and the mould attached to a crucible as before described. When cast, and the mould broken away, the chain comes out inflexible, being attached to a rod which runs round where the groove was cut. This is broken off, and the chain is complete.

Having been consulted respecting the trades to be represented in the Indian Courts of the Colonial and Indian Exhibition, he recommended amongst others a good brassfounder to be sent. Dr. Tyler, who was charged with the collection of these artisans, engaged one of the best he could find, but up to the present the foundry is not in working order.

One of these combined crucible moulds was submitted for inspection, with fragments of another, also a cast curb chain anklet ; the author concluding by thanking the members for this opportunity of publishing an interesting process.

A NEW SPECTROMETER

IN equipping the Physical Laboratory of University College, Dundee, I felt considerable difficulty in deciding on a spectrometer for accurate work ; it was easy to get a simple instrument for qualitative experiments and rough quantitative work, but it was only after consulting several friends and communicating with two or three firms that about two years ago I wrote to Mr. Hilger, in the hope that from him we might obtain an instrument capable of working to as high a degree of accuracy as would enable our best students in the laboratory to do advanced work. Considering that a second of arc is by no means an unusual limit of error in angular measurement, and that it is of the order 1 : 1,000,000, the whole circle being unit, we thought that while further capability in reading power would be more than counterbalanced by various indeterminate errors, yet it should be possible to obtain this accuracy with a suitable instrument.

Prof. Liveing was kind enough to give us valuable information about one of his own instruments, of which the plans were sent to us by Mr. Hilger for inspection ; and Mr. Capstick and I finally decided to ask Mr. Hilger whether he could not arrange two microscopes on the instrument in place of the one which Prof. Liveing's has.

As a consequence, Mr. Hilger presented suggestions for a spectrometer which is now in this college, and is capable of reading directly to one second of arc and yielding reliable results. Its construction is very simple. The collimator stands on a heavy pillar by itself ; and the circle, divided to five minutes of arc on a ring 15 inches in diameter, with six radial spokes, is carried on another pillar. The telescope, counterpoised, turns on the same axis, but does not touch the circle at any point ; and the reading is managed as follows : from the telescope-bearing a double girder with a semicircular plan tied across its diameter by tubes of brass stretches horizontally above the semi-circumference of the divided circle ; to this girder are fixed, at its ends,